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that each of Pritchard's results depends on four plates, and not on a single plate as is there assumed.

The amended conclusion of my criticism should therefore be that it requires measures of thirty-two Oxford plates—even if they were free from systematic error—to have the same weight in the determination of parallax as one Heliometer observation.

The Oxford Photographic Determinations of Stellar Parallax. Further Reply to Sir David Gill. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

Sir David Gill kindly sent me a copy of the above reply, and it may be convenient to have my rejoinder read to the Society at the same time as his paper.

In his volume on stellar parallax Sir David Gill gave three reasons for regarding the Oxford determinations as not "of proved value," which may be briefly stated as follows :

(1) That Pritchard gave separate results for parallax from two comparison stars *a* and *b*, although the distance *a b* had been used to correct each measure for scale-value.

(2) The "chromatic dispersion" objection.

(3) That the figures published by Pritchard gave evidence of distortion of the film, sufficient to render the observations of small value.

In a former paper (*Monthly Notices*, lxi. 5) I considered the third objection only, for as it deals with an actual matter of fact it seemed to me by far the most important. Sir David Gill's reply is chiefly in his postscript, which I will consider presently.

As regards the first and second points which Sir David now pronounces the more important, I am again at variance with him. To the first he has himself supplied "the obvious answer." Surely there is no harm done in giving the results separately ? If they are not independent the mean can be taken as Sir David Gill says ; and to exhibit them separately has the advantage of showing that there is no numerical mistake, if nothing more.

As regards the second point (between which and the first Sir David Gill traces some connection which I fail to see), it is quite true that Pritchard made no experiments on the effect of chromatic dispersion. It is equally true that Sir David Gill had made none himself at that time, though he had also published parallax work.

The discussion about chromatic dispersion of the atmosphere has come up since then. Indeed, it was not until 1898, when Pritchard had been dead five years, that Sir David Gill initiated

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observations to test its reality as a disturbing cause on heliometer observations; and when these were made the whole thing was found to be something very like a mare's nest. Sir David Gill says in his Introduction (*Cape Annals*, vol. viii. pt. II., p. viii.): "I may state at once that the results of these observations go to show that the observer in the process of measurement, unconsciously superposes, *not* the most brilliant points of the two short stellar spectra formed by the chromatic dispersion of the atmosphere, but the two similarly coloured parts of these spectra, and thus the effects of chromatic dispersion are entirely eliminated."

Now here we have in the same breath two distinct things—an observed fact and Sir David Gill's inference from it. The *observed fact* is *merely* that the observations are not affected. The inference as to *why* this happens is not absolutely proved. Atmospheric chromatic dispersion is represented by a quantity $\Delta\beta$, the value of which is as yet undetermined. Sir David Gill has "suggested a method of determining the degree of redness of a star" (he gives, however, the wrong reference, and I have not been able certainly to identify the right reference; in *Cape Annals*, vol. viii. pt. II., p. 125B the reference is given as *Monthly Notices*, 1898 December, p. 98. It is, perhaps, 1897 December, p. 68?), but he has apparently got no further than suggesting a *method for determining* $\Delta\beta$. Now I submit that until we know more about the value of $\Delta\beta$ there are two alternative inferences (at least) from the fact that the observations made on δ *Sagittarii* with a heliometer show no effect. One possibility is that $\Delta\beta=0$ to the order required—that chromatic dispersion does not sensibly affect such observations either heliometric or photographic. Sir David Gill prefers to say: We know that $\Delta\beta$ must be sensible, and as it does not apparently affect heliometer observations there must be something in the method of observation which eliminates it. His experience with the heliometer entitles this opinion to the greatest respect, but we must be careful to remember that it is as yet *only* an opinion, and that there are other ways of explaining the observed fact. Therefore it is not quite "obvious" * that photographic observations will be affected just as surely as heliometer observations are not. The experiment has still to be tried, and it is, to say the least, quite possible that atmospheric chromatic dispersion affects photographic parallaxes as little as heliometer parallaxes. Theorising beforehand has been proved misleading in the latter case (although not until a year or two ago) and may be just as misleading in the former.

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* Sir David Gill writes: "It is obvious, for example, that any displacement of the apparent centre of the star's disc by chromatic dispersion of the atmosphere will, especially in the case of a bright star, be recorded on the photographic plate, and will not be eliminated as in the heliometer observations, by the observer superposing the similarly coloured parts of the spectra under observation." We have here several assumptions which are by no means obvious.

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With regard to the postscript in which Sir David Gill deals with the third point, it is almost sufficient for my purpose to compare his original statement with that which he now substitutes. The original statement was: "Throughout the series there is little evidence of systematic agreement even in the signs of the scale-value corrections." That of his postscript is now: "Even as matters stand we find that out of 87 pairs of scale-value corrections, the components of which should have the same sign, no less than 24 have opposite signs." (The 24 should be 20: I have counted very carefully both in the original table and in my rearrangement of it, *Monthly Notices*, lxi. p. 310.)

Now if Sir David Gill regards these statements as equivalent we have reached the point at which this discussion may stop—where we must "agree to differ." To me there is such an essential distinction between them that I should have expected from him something like a withdrawal of the original statement. With a series of small quantities all near zero a certain number of accidental differences of sign is inevitable; and of the 20 pairs the following 8 for example may be regarded as sensibly zero in both members:—

	″	″
Aug. 30	+0·041	−0·038
Oct. 2	−0·003	+0·070
Oct. 21	−0·046	+0·004
Nov. 17	+0·031	−0·035
Nov. 29	+0·016	−0·002
Dec. 2	−0·047	+0·003
Apr. 26	−0·048	+0·035
Apr. 29	−0·011	+0·061

Of the remaining 12 five belong to the 11 observations at the beginning of the series where I have already explained that there is an error [I may, perhaps, reassure Sir David Gill as to the recurrence of such errors: from August 20 the mirror was not changed], which leaves only seven sensible differences of sign in the 76 observations after the work was satisfactorily begun.

A word as to the phrases "when the instantaneous scale-value is *grossly* different from the mean;" "by focussing *badly* enough;" "a sufficiently *erroneous* mean scale-value." Sir David Gill uses these phrases to characterise differences of scale-value amounting in extreme cases during a year to 0″·6 in 1000″, or ·0006 of the unit. May I ask him to examine the scale-values of the Astrographic Chart plates which he is now taking and measuring at the Cape? Or will he perhaps kindly look at figures already published, say those in *Monthly Notices*, lv. pp. 62 and 63? Here we see compared the scale-values on four plates taken at Greenwich on the *same* night—not of the same region, but of regions overlapping sufficiently well to give a good

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comparison—and the scale-values, indicated by the constants a and e, differ to fully half the extent shown in Pritchard's results. It may be remarked that the mere *change of measurer*, *keeping the plates the same*, can give an apparent change of scale-value from +·00022 to −·00017 (see the values of e for plates 2059 and 2057 *loc. cit.*). In Pritchard's Table XIII. these quantities would appear as +0″·22 and −0″·17. If, then, we get such accidental differences for plates taken within a couple of hours on the same night with a refractor (which is known to be more manageable than a reflector), after all the precautions of agate-stops &c. which were devised by the united wisdom of an international conference, and *when* 30 *or* 40 *stars are used*; it seems to me that Pritchard's range of differences for plates taken all the year round with a reflector, as pioneer work, *and with only two stars measured*, is not excessive; and that Sir David Gill's epithets are quite out of place.

I pass to the new examination of Table XIII. undertaken by Sir David Gill in his postscript. He finds the probable error of a measure of distance from Table XIII. and compares it with that found elsewhere by Pritchard (p. 65 of Pritchard's *Memoirs* is better than the scattered references quoted by Sir D. Gill). I remark in reply :—

(1) If the eleven observations May 30 to July 1, in which there is an error (as stated in my former paper) be omitted, the sum of the squares is reduced from 2″·81 to 1″·62; and (following Sir David Gill's further work) the probable error of a single distance ab or cd would be ± 0″·147 instead of ± 0″·190. If we are trying merely to make out a strong case against Pritchard we might of course insist on retaining all the printed figures; but if we are really trying to find out the truth of the matter, it is better to exclude figures known to be erroneous.

(2) Sir David Gill assumes apparently that the probable error of a distance is independent of the magnitude of the distance measured. It seems more likely that it is greater for large distances than for small; and this view is confirmed on reference to the figures. Pritchard gives in various tables the "average deviation" of the measured distances; and, on adding up corresponding tables, we find that the average deviation for the half-distances from the central star to the four stars a, b, c, d is about 0·83 times [*] that for the whole distances ab and cd; and this in spite of the fact that "the smaller probable error" (to quote Sir David Gill's words) "is derived from measures between two images, one of which is that of the large and less

[*] In Tables XX., XXII., VIII., X., XVII., XV., V., II., XIV., I., Pritchard gives comparable "average deviations" for distances of 955″, 963″, 1002″, 1022″, 1107″, 1115″, 1360″, 1380″, 2065″, and 2380″. The means for the first eight tables are much the same, viz. 0″·183, 0″·175, 0″·191, 0″·199, 0″·183, 0″·189, 0″·185, 0″·185, mean of all 0″·186. The means for the last two are 0″·219 and 0″·229, mean of both 0″·224. Ratio of 0″·186 to 0″·224 is 0·83.

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sharply defined disc of the principal star, whilst the larger is derived from measures between the smaller and sharper discs of the comparison stars." Indeed, by using this factor 0·83 as it stands, we shall not only reduce the probable error for a distance of 2200″ so as to be properly comparable with one of half that distance, but we shall at the same time compensate directly the unknown disturbance of the results due to the large disc of the central star, which Sir David Gill leaves as an outstanding balance in his own favour. This factor 0·83 then reduces our probable error found from the comparison of *ab* and *cd* still further from 0″·147 to 0″·122, and the probable effect of the "additional fault or cause of error" is reduced from $\sqrt{(0''\cdot19)^2-(0''\cdot10)^2}=\pm0''\cdot16$ to $\sqrt{(0''\cdot122)^2-(0''\cdot100)^2}=\pm0''\cdot07$ which is a sort of quantity one might quite reasonably expect.

(3) Accepting this remainder as a real difference, I must still demur to Sir David Gill's statement that I have written as if this difference (he calls it an "error") were non-existent. On the other hand, I spent some paragraphs in considering possible causes for it, which he has quoted. When I spoke of an "imaginary fault" I was referring to quite another matter, viz. the fault originally assigned to the observations by Sir David Gill in saying that "there is little evidence of systematic agreement even in the signs." I have already pointed out that his revised statement ("we find that out of 87 pairs no less than 20 have opposite signs") is very different from the former; and there is the same wide difference between the "fault" I called imaginary and the discrepancy, now reduced to an amount not surprising, for which I have already assigned three other possible contributing causes at least. And, if Sir David Gill wishes, I will admit as a possible fourth the "distortion of the film." I have called it an "imaginary cause," and will still do so; for its sensible existence has still to be proved. That it cannot be the sole or even the chief cause of the discrepancy under consideration follows from the fact to which Sir David Gill himself draws attention, viz. that "there are long periods in which the differences of scale value have the same sign." It will require some ingenuity to explain how distortion of the film can act in this way.

Of the other three causes suggested by me let me take first that which enables Sir David Gill to contribute to the gaiety of nations—I allude to the "freezing and perspiring" paragraphs. It seems sad to have to knock the bottom out of a joke; for we get so few in astronomy, especially in such serious work as parallax work. But the whole point of this joke lies in its coefficient, and I cannot accept the coefficient. First of all, where does Sir David Gill's 0″·19 come from? Surely he means $\sqrt{(0''\cdot19)^2-(0''\cdot10)^2}$ or 0″·16 as above? He would probably have corrected this in proof (as also the 24 for 20 earlier mentioned. I should have gladly corrected these slips for the distant author, but he has rendered this course impracticable by

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laying such stress on the figures), and then he would have contracted his range of 58° F. to 49° F. But a further contraction is necessitated by the little revision of figures I have sketched above—bringing the 49° down to 21°. I must finally submit that temperature is by no means called upon to explain the *whole* discrepancy; half or a third will be quite a respectable contribution, and so we get down to 10° or 7° perhaps, which is quite sober earnest.

Next as regards the tilt of the plate: "If the mounting of the plate-holder was reasonably rigid it is improbable that this adjustment would change sensibly between one night and the next for plates taken nearly at the same hour-angle." Sir David Gill is making no allowance for the variable curvature of the plate, which is an important factor in these matters. Once again I would refer him with confidence to his own results or to such published figures as have been already quoted; and I will ask him to remember that the mounting of a plate-holder in a reflector cannot be so rigid as in a refractor. Finally, my suggestion about the curvature of the mirror he calls a mistake. Possibly; but I am not convinced by what he says. The point is not an easy one, and actual experiment will be the best test; but I will venture to put down the way in which it appears to me. If scale value has any uniform meaning, we may suppose the mirror replaced by a pinhole, from which the rays diverge to the plate. The scale value will then depend simply on the distance of this pinhole from the plate. Now *where* is this pinhole? Is it absolutely *at* the surface of the mirror? It must be close to it, but I do not feel at all sure that it is not at a small distance from it. We can scarcely decide by simple theory; for the measurement of images and even the actual meaning of the word "focus" are matters of compromise, on lines only imperfectly worked out theoretically; and the position of this hypothetical pinhole depends on them. Hence, speaking tentatively, I would point out that if the place of this representative pinhole is not rigidly *at* the surface of the mirror, its distance from it must depend on the curvature (there is nothing else for it to depend on); consequently, if the curvature is different in different directions we shall have different representative pinholes for these directions, *i.e.* different scale values. Experiments would settle the point, but it is not easy to settle it theoretically. To prevent possible misunderstanding, I may say that I consider all the following points to be proper subjects for experiment:

(1) Photographic distortion of the film.
(2) Atmospheric chromatic dispersion.
(3) Effect of flexure of mirror on scale value.

Of none of these have I intentionally denied the *possible* existence; I will even affirm their existence. But to what extent? The whole point is, What is their magnitude?

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The discussion has already reached a greater length than I anticipated; for, as I have already said, it is not my intention to undertake a systematic re-examination of Pritchard's work. Starting such work now one would do it differently perhaps; we have had ten years' experience in photographic measurement since Pritchard began. But that his work can be dismissed in the cursory manner of Sir David Gill's Introduction I do not believe. It is at least worthy of careful examination. For one thing it is (so far as I know) even yet the only considerable research on stellar parallax by photography actually carried out. Many peeple have talked of doing such work before and since, but as compared with talk, an actual piece of work done is entitled to a measure of respect.

The Normal Equations that arise in the usual Schemes of Observation for Division Errors and their Solutions. By P. H. Cowell, M.A.

Notation.

The circle of 360° is supposed divided into n arcs of $\frac{360^\circ}{n}$ each; and the error of the division $r\frac{360^\circ}{n}+\text{A}^\circ$ is denoted by x_r, where r takes all the values 1, 2 . . . n. At the first setting of the circle the $s+1$th division at the $\text{A}+(s+1)\frac{360}{n}$th degree is supposed to be under the sth microscope. In some cases s will take all integral values from 1 to n, but in other cases s will only take some of these values. Between the settings the graduated circle is supposed to be turned in such a way that the $s+t$th division at the $\text{A}+(s+t)\frac{360}{n}$th degree is under microscope s. The series of observations will be complete when there have been n settings of the circle, but in order to eliminate time changes it is usual to at once repeat the observations in the reverse order. Any uniform change is eliminated either by taking the mean of corresponding readings, that is to say, readings equidistant from the middle of the double series, or preferably by solving each single series separately, and taking the mean of the final results, whose discordance indicates the amplitude of the changes that may have occurred.

It is henceforth assumed that a single series of observations is being dealt with.

The division error x_r is defined as the amount by which the circle appears to read too large in consequence of division error. The sth microscope is supposed subject to an error of position